

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Photograph of the Nebula in Orion

DURING the night of September 30 I succeeded in photographing the bright part of the nebula in Orion in the vicinity of the trapezium. The photographs show the mottled appearance of this region distinctly. I intend shortly to publish a detailed description of the negatives. They were taken by the aid of a triple objective of eleven inches aperture made by Alvan Clark and Sons, and corrected especially for the photographic rays. The equatorial stand and driving-clock I constructed myself. The exposure was fifty minutes.

HENRY DRAPER

New York, October 2

An Annelidan Entozoon

WHILE examining the intestinal tract of *Megaderma frons* from the Gold Coast, I found coiled up spirally and adhering to the wall of the lower part of the ileum a small parasite about half an inch in length. On placing this under the microscope I was much surprised to find that it belonged to a class of worms (*Annelida*), none of the species of which have hitherto been known as Entozoa, and further that I was unable to refer it to any of the orders of that class.

On showing it to Dr. J. D. Macdonald, F.R.S., he quite agreed with my opinion that it represents a new order of Annelids, and is moreover disposed to consider it as a connecting-link, hitherto wanting, between the *Chaetopoda* and the true leeches.

The specimen in question is about half an inch in length without distinct segmentation, except what is indicated by the perfectly regular disposition of the cephalo-somatic appendages—seventy-three pairs, extending from the anterior almost to the posterior extremity of the body—whereof those occupying the anterior attenuated fourth of the body are fin-like lamellae, apparently branchial, with a single unarmed mouth not provided with a proboscis, with the intestine spirally coiled round the ovarian tube and terminating inferiorly at the posterior extremity of the body.

Megaderma frons, the host of this remarkable annelid, is a species of bat of very peculiar aspect, which is apparently widely distributed throughout and restricted to the tropical parts of the Ethiopian region. It belongs to a genus whereof one of the species at least is known to suck the blood of smaller bats, which it captures on the wing (see my "Monograph of the Asiatic Chiroptera," p. 77), and as all the species closely resemble one another in structure, it is exceedingly probable that they have all much the same habits.

Although I found remains of insects in the intestinal canal of the specimen from which the above-noticed parasite was taken, yet there was also mixed up with them a large quantity of hair, not from its own body, but evidently (judging from its microscopic structure) that of some other bat on which very likely it had been feeding. It is also worthy of notice that the intestine of the parasite is filled with a reddish matter like the remains of blood.

I have handed over this very interesting specimen to Dr. Macdonald, who will shortly publish a full description of it with figures.

G. E. DOBSON

Royal Victoria Hospital, Netley, October 7

Sounds made by Ants

FROM the very interesting remarks lately made by Sir John Lubbock regarding the habits and capabilities of ants, I gather that he seems to consider them as a silent group. The modes of producing sounds among insects are as various as beautiful, whether by internal or external agency. As a rule the larger animals produce sound by internal means, *i.e.* voice, and insects by some external means.

Among ants I know of two varieties or distinct kinds, a black and brown, that make a concerted noise loud enough to be heard

by a human being at twenty or thirty feet distance, and which sound is produced by each ant scraping the horny apex of the abdomen three times in rapid succession on the dry crisp leaves of which the nest is usually composed.

The noise made by a single ant is sufficiently loud to be heard on a very dry leaf if attention is directed to it, and no doubt by this means of a vibrating medium they can without special auditory organs communicate with each other. I had the honour of first discovering that the great *Mygale stridulans* made a noise; the apparatus by which it was produced was discovered and fully described by Mr. J. Wood Mason of the Indian Museum, and I should be glad if I am the means of making a similar discovery regarding ants. White ants (so-called) make a noise which is audible—if put on crisp paper—by suddenly shaking the whole body, and seem to warn each other by this means.

Sapakati, Sibsagor, Asam, August 20

S. E. PEAL

Faraday Exhibiting Ghosts

MR. J. INNES ROGERS' communication on a "Spectre of the Brocken at home" reminds me of a passage in Dr. Bence Jones's "Memoir of Faraday," vol. i. p. 422.

Faraday's niece, Miss Reid, thus writes: "One evening a thick white mist rose and completely hid everything before us. About ten o'clock my uncle called me into his room to see a spectre. He placed the candle behind us as we stood at the window, and there, opposite to us, appeared two gigantic shadowy beings, who imitated every movement that we made."

Ardchapel, N.B., October 16

W. S.

Ice under Pressure

IN reply to C. A. M.'s letter of last week I would make the following remarks:—Ice is not an exceptional substance, for mercuric chloride has also given experimentally the same results, and though I have not yet had the opportunity of submitting other substances to the same conditions, yet I conclude from other experiments that all the bodies which I have so far investigated, and which are of the most varied description, will also exhibit the same phenomenon. As I have not yet published my detailed results, I do not wish at present to enter more fully into the subject, but I may say that the influence of pressure in the present case is not of the same kind as that referred to by C. A. M. as occurring in the text-book named, for the following amongst other reasons. From Prof. Thompson's prediction and Sir Wm. Thomson's experiments it resulted that the melting-point of ice is lowered by pressure, and lowered in proportion to the pressure, whereas in my experiments, at any rate so far as I have at present seen, we do not vary the melting-point by diminishing the pressure, but we prevent the substance from melting at all. If the pressure be increased even but slightly above the critical pressure, the ice melts at its ordinary melting-point. The influence of pressure in this case is not one of degree varying with the amount by which the pressure is reduced. The two cases are, I consider, entirely different, and are not contradictions. Similar remarks would probably apply to paraffin and spermaceti, though these are bodies which have not come within the range of my experiments.

Firth College, Sheffield, October 6

THOS. CARNELLEY

A Peat Bed in the Drift at Oldham

IN NATURE, vol. xxii. p. 460, there is a letter by Mr. Jas. Nield, giving an interesting description of unique, or nearly unique, appearances in the boulder clay near Oldham. It appears that this glacial deposit has one or more beds of peat, or fragments of peat, intercalated along with it at various depths, leading to the inference that the clay had been stirred up and the fragments of peat had in some manner been mixed with it. That peat bogs, or surface black peaty mould, had existed at no great distance is a conclusion forced upon us, and that the action of ice and snow, probably during a submergence, had mashed up the clay and distributed the peat amongst it. The boulder clay, and the scratched mountain sides, and the travelled fragments of rock, do not extend over the whole of England. It used to be said by geologists that the effects of a severe Arctic climate could not be detected south of a line drawn across the country from London to Bristol; by which it was inferred that all the land north of that line had been under water, subject to the influences of snow

and floating icebergs, and all the country south of it above water and clear of those influences. Since then the large granite boulder on the shore of Barnstaple Bay, estimated to weigh ten tons, has been brought more prominently under our notice by Mr. W. Pengelly (*Trans. Dev. Assoc.* vi. 211), and several others by Mr. T. M. Hall (*Id.* xi. 429), discovered by excavation. All these are travelled blocks, and probably ice-borne. Many attempts have been made by ardent and intelligent students of late years to detect proofs of glacial action further south, and even to the shores of the British Channel, but hitherto with doubtful success. There lies on the greensand of Haldon, near Exeter, and on the Blackdown Hills, stretching away towards the south-east corner of the county of Devon, a stratum of tough yellow clay full of white flints, mostly angular. About Haldon and eastward over Pitminster and Churchstanton, many white quartz rounded pebbles, foreign to the accompanying beds, are met with. Farther south, between Honiton and the sea, this stratum of flints and clay in some places is seen to be from forty to fifty feet thick, and the best section of it is in the gravel pits near the cliff on Peak Hill, on the west of Sidmouth. By some persons this deposit has been regarded as the thinned-out edge of the plastic clay formation, containing the remaining flints of the washed-out chalk, still found more perfect at Beer Head, a few miles east. Whether it was this, or whether it was a boulder clay, so called, it is well to remark that, though thickest on the flat tops of the hills, it seems to lap down over their sides, as if it had been deposited after the valleys and the elevations had come to their present conformation; and at two places at least to be visible in the valley of Sidmouth—one under the great blocks of breccia in the orchard near the brook on the Boomer or Boughmoor Estate, and the other on a subordinate hill in a grass field, at about 200 yards from Jenny Pine's Corner, walking down the new road towards Cotmaton, and on the right-hand side. Most of this latter patch of clay and flints was dug away two or three years ago to assist in forming the new road.

When engaged in making certain trenches and excavations on the top of Salcombe Hill in 1879 for archaeological purposes (see *Proceedings Soc. Antiq. Lon.* viii. 209) it appeared that the yellow clay, to the depth of two or three feet, was not so much encumbered with flints as deeper down. But whilst thus engaged, what struck me as rather strange was that numerous fragments of black peat were more or less generally but irregularly distributed through the upper two feet; and bearing in mind Mr. Nield's letter, I have in my foregoing remarks been trying to lead up to this point. The cases may not be similar, but they are worth comparing. The land on the top of the hill at this place still bears its wild growth of heath and furze, and has never been subjected to the plough or to cultivation of any sort; so that the clay has not been disturbed by the hand of man. It is too soon to say that this capping of clay and flints is of glacial origin; but some of the indications that have suggested the idea may be observed in the section in the gravel pits on Peak Hill, especially when fresh dug down. They are: (1) that no horizontal bedding is visible, as there would be if the deposit had been made in a large body of undisturbed water; (2) That, on the contrary, waving and distorted lines are sometimes very plain, one instance of which I carefully sketched and coloured in January, 1875; (3) and that the long axes of the embedded flints do not as a rule lie horizontally, as they would necessarily do if they had settled at the bottom of a sea or pond, just as an egg will lie on its side, and not on its point, but they are distributed through the soft mass at all angles, as raisins lie in a pudding that has been kneaded up together.

My object in this communication has been merely to compare the case of the peat mixed with the clay on Salcombe Hill with the somewhat similar case occurring near Oldham.

P. O. HUTCHINSON

Old Chancel, Sidmouth, Devon, October 4

IN NATURE, vol. xxii. p. 511, I find a letter from Mr. G. H. Morton, in which he expresses an opinion contrary to that expressed by me (vol. xxii. p. 460), as to the age of the "peat bed in the drift of Oldham." The section therein alluded to is fairly described by him, but I am surprised that he should for an instant entertain the belief that the clay "has simply slipped down off the sand on to the surface of the peat at a lower level." Had the clay slipped down we ought to have been able to see some indications of the conjectured displacement. Let me say, however, that during my repeated visits to the place and my examinations of the section I have utterly failed to perceive any

trace of such indications, and, moreover, I do not remember that one person out of some scores who have in presence of the section pointed out to me the slightest appearance of disturbance. There is not a broken or crumpled line in the whole section.

The peat bed, and indeed the whole of the section, is now, I am sorry to say, covered up; but in and about Oldham we have a large area covered by what I believe to be typical beds of the "Middle" and the bottom of the Upper Drift—alternations of gravel, pebbles, fine and coarse sand, the latter showing lines of "current bedding," and occasionally clay with boulders—in which many similar sections, but wanting in the peat, of course, may be seen, and in which the position and surroundings of the beds quite forbid the possibility of "slipping." The idea of the upper clay "having been excavated and thrown down" is, I think, too improbable to be seriously entertained, seeing that the surface-soil and subsoil on the top of it are of the usual thickness common to the neighbourhood.

The "blue silt" alluded to by Mr. Morton as giving strength to his suspicions, I can assure him is one of the supports upon which I rest my opinion that there has been no disturbance. Do I understand him to mean that the silt is the result of the washings of some passing stream? If so, let me recommend him to visit the railway cutting across the large peat bog a few miles from here, and known as the "Ashton Moss," where he will find, at the bottom of a bed of recent peat, of from two to three yards in thickness, a thicker, but in every other respect a similar band of blue silt, upon which the peat rests throughout the length of the whole cutting. This silt seems to have its equivalent in the "floor clay" which accompanies our seams of coal. I believe that the removal of so much of the peat bed and drift deposits from the face of the excavation as has already taken place has served all the purposes of the "few hours digging at a right angle to the present exposure," suggested by Mr. Morton.

Perhaps a more complete acquaintance with the Oldham drift beds would bring Mr. Morton nearer to my way of thinking. I shall be glad to see him here again, and to assist him in making a wider, and more thorough examination of them.

29, Radcliffe Street, Oldham, October 7 JAMES NIELD

Temperature of the Breath

MR. McNALLY has, it appears to me, missed the point of my observations on this subject.

His own experiments, though they show a temperature obtained by breathing on a thermometer through silk, wool, and linen, considerably above the accepted temperature of the breath, are by no means an exact repetition of mine. He only breathed through four folds of the material for three minutes. I breathed through a much greater amount of material and for a longer time, viz., twenty to thirty folds tightly encircling the thermometer bulb for five minutes.

The temperatures I obtained were very much higher than those observed by your correspondent. Thus on a warm summer day the temperature obtained on rising in the morning before dressing and before eating was 106°. In the afternoon, after playing a game of golf, when returning home by rail with all the windows open, the temperature observed was 107°. The same day, after dinner (without alcoholic stimulants), the thermometer rose to 108° when breathed on in the way described. The temperature of the air that day averaged 70°. Since then I have not obtained a higher temperature than 107°·5.

These temperatures were obtained by breathing through a silk pocket-handkerchief tightly rolled round the thermometer, but I have obtained temperatures nearly as high when the thermometer was wrapped up in cotton or woollen stuff.

Mr. McNally asserts that the explanation suggested by my friend that the high temperature thus obtained was owing to the heat evolved by the condensation of the aqueous vapour contained in the breath is "undoubtedly correct," but he gives no answer to the obvious objection to this explanation, viz., that if the real temperature of the breath be, as stated in physiological works, 95° to 97°, condensation of the aqueous vapour in it would only take place as long as the material through which it is propelled was of lower temperature than the breath. When the material attained a higher temperature than 97° the aqueous vapour, in place of being condensed, and thus evolving heat, would be still further evaporated, and hence be a cause of reduction of temperature.

The fact that woollen clothing prevents chill after exercise has